

SCIENCE

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THE NORIAN ROCKS OF CANADA.

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AN important advance has been made in American Archæan geology by the publication of a memoir, by Professor Frank D. Adams of McGill College, Montreal, on the Norian or Upper Laurentian rocks of Canada.¹ The memoir embraces the results of five seasons' field-work conducted for the Geological Survey of Canada, together with an exhaustive and masterly petrographical investigation in the laboratory of the rocks constituting the Norian terranes. A full bibliography of these interesting rocks accompanies the memoir; also a map showing their known distribution, and a table of twenty-four analyses compiled from various sources. Thoroughness of research and soundness of judgment characterize the work throughout, and all interested in Archæan geology or in general petrography will welcome it as one of the sure steps which make for scientific progress. The author introduces his subject by a general presentation of the geology of the Laurentian, in which he gives a brief history of the growth of our knowledge and theories of these rocks, and discusses their nomenclature, both geological and petrographical. In the latter connection he takes occasion to describe, in general terms, the rock of which the Norian is prevalently constituted, and makes good its claim to be regarded as a distinct petrographical type, the characteristic of which is the preponderance of plagioclase to the partial or entire exclusion of the ferro-magnesian silicates. For this type of rock he uses the term "anorthosite," in accordance with a usage which has long been in vogue among the Canadian geologists. The anorthosites are regarded as belonging to the gabbro family, constituting one extreme of a graded series, of which pyroxmites and other granular rocks, rich in ferro-magnesian minerals to the practical exclusion of feldspar, are the other extreme, while common gabbros, norites, etc., are the middle members.

The various regions occupied by these anorthosites are described, and the petrographical and geological relations are discussed in detail. Their occurrence appears to have a peculiar relation to the so-called Archæan nucleus of the continent. So far as explorations serve us, they are only known to occur on the periphery of this great "Canadian shield," along the line of the St. Lawrence drainage, from near its source to the Straits of Belle Isle, and again farther north on the Labrador coast. Professor Adams calls attention to the seeming analogy between this distribution and the modern distribution of volcanoes on the periphery of the continents. The more important anorthosite occurrences, both by reason of their great extent and for the greater study that has been given to them, are (1) the Morin region, north of Montreal, and (2) the Saguenay region, which is the largest known. The Morin mass has a diameter of 37 miles and an area of 990 square miles. It is surrounded by the gneisses, crystalline limestones, quartzites, etc., of the Grenville series (Laurentian), through which it is clearly eruptive. It holds included in it blocks of the surrounding gneiss, and the latter is traversed by apophyses from the main mass; and at places distinct evidence of an alteration contact-zone is observable. The deeply-worn surface of both the Laurentian and the invading anorthosite is overlaid by flat-lying, unaltered Cambrian strata. The age of the anorthosite is thus limited as Post-Laurentian and Pre-Cambrian. Petrographically, the anorthosites are characterized by a peculiar cata-

clastic structure, which is most pronounced where the rock mass evinces a schistose or foliated structure. This cataclastic structure is ascribed to pressure acting on the mass and deforming it while it was yet deep in the crust of the earth, and at a temperature probably near its fusion point. In addition to the dominant mineral plagioclase, many other rock-forming minerals occur, such as augite, hypsthene, ilmenite, hornblende, biotite, etc. Most of these play a very subordinate rôle as normal constituents, while others are rare accessories; and some are secondary or decomposition products. Certain layers of anorthosite occur intercalated with the gneiss and crystalline limestone of the surrounding Grenville series. These layers of apparently interbedded anorthosites vary in thickness from one yard to several hundred yards, and in length from half a mile to eight miles. The apparent interbedding is due to the intrusion of the anorthosite within the strata of the Grenville series.

The Saguenay region presents a mass of anorthosite of still more extensive proportions, occupying not less than 5,800 square miles. The anorthosite of this region is essentially similar to that of the Morin region, both petrographically and in its relations to the Laurentian and to the base of the Palæozoic. There are, however, some differences in petrographical detail; one of these being the common occurrence of olivine among the ferro-magnesian constituents. The Laurentian gneiss of the surrounding region corresponds rather to the Ottawa gneiss than to the Grenville series, and the contact phenomena on the periphery of the anorthosite mass are more confused than in the case of the Morin mass.

The other localities where occurrences of anorthosite rocks are described are: In Labrador, in Newfoundland, on the north side of the St. Lawrence River and Gulf, in the State of New York, and on the east coast of Lake Huron. To this list of localities the present writer may be permitted to add northern New Jersey and the northwest coast of Lake Superior, where he has met with anorthosite rocks in the field.

Among the more important results of Professor Adams's work may be mentioned:—

1. The clear recognition, as plutonic eruptive formations, of rock masses, which, being petrographically and geologically units, have each an enormous extent. The Morin mass occupies nearly 1,000 square miles, and the Saguenay mass is at least about six times that area. For such irruptive plutonic masses Suess has proposed the designation "batholite," and it will facilitate discussion to use that term. The recognition and description of the laccolitic type of mountain structure by Gilbert has been prolific of very fruitful results. The fact that there are such structures was so ably presented by their discoverer that it was warmly and generally received, and it has become an important element in discussions of orogeny. The fact that there *are* batholites has not had so vigorous a presentation, and the important rôle which they play in the structure of the earth's crust has not received the recognition which its importance merits. The able account which Professor Adams gives us of these great batholites of anorthosite is, therefore, a welcome addition to our knowledge of such structures. The present writer has elsewhere endeavored to call attention to the existence of granitic batholites in the Rainy Lake region, which are comparable in size to the anorthosite areas of Quebec; and it seems to him that a more general appreciation of the importance of these great igneous masses would simplify and advance our discussion of certain problems of tectonic geology, metamorphism, and geognosy. If it be a fact, as Professor Adams and the writer have shown for two distinct regions, that immense masses of igneous matter, ranging from 25 to 100 miles or more in diameter, have invaded the crust from below, dis-

¹ Ueber das Norian oder Ober-Laurentian von Canada. Stuttgart, 1893.

placing or absorbing it, is it not likely that such an event has occurred frequently and in many different parts of the earth's crust? Geological records are, indeed, full of suggestions that such is the case. Yet in all our current discussions of orogeny, epeirogeny, and regional metamorphism, how little is this factor in the problem considered? What part has the development of the Nova Scotia batholite played in the folding of the Cambrian strata of that province, in their metamorphism and in their becoming charged with gold? What part has the development of the great batholite of the Sierra Nevada played in the folding and metamorphism of the earlier Mesozoic rocks of California and in their becoming charged with gold? What part has the development of the great British Columbian batholite played in the folding and metamorphism of the earlier Mesozoic strata of the west coast of that province? Was the British Columbian batholite synchronous in its development with the Sierra Nevada batholite? Are they separate and distinct affairs, or are they simply geographically separate manifestations of one stupendous process of crust development? In either case has not the exposure by denudation of these great batholites and their intrusive relations to the surrounding terranes practically reproduced the conditions which we find in the Archæan terranes of the Canadian plateau? These are a few of the questions which can only be profitably discussed when the batholite is recognized as a much larger element in tectonic geology than the dyke, the neck, the boss, the sill, or the laccolite. Batholites abound. Why should they not be recognized?

2. A second important result is the immense simplification which is effected in Archæan geology in the Canadian territory, where most questions of that ilk must find their final solution. Hitherto the Norian rocks have been classed as part of a supposed system of metamorphic sedimentary strata known as the Laurentian. This system was divided by Logan into an upper and a lower division, the latter being sub-divided into two parts, viz.: the Grenville series and the Ottawa gneiss, so that his scheme stood thus:—

Norian series = Upper Laurentian
 Grenville series = Upper division } of Lower Laurentian.
 Ottawa gneiss = Lower division }

The recognition of the irruptive character and post-Grenville age of the Norian rocks is a great gain, and reduces the Laurentian system to two members. The simplification thus effected suggests to the present writer still other possibilities in the same direction. It seems probable that the Grenville is a profoundly metamorphosed series of sedimentary strata. Its bedded character and the fact of its being composed of strata of limestone, quartzite, iron ore, graphite, etc., in addition to the gneisses, favor this view. The Ottawa gneiss, on the other hand, has a very different character. There are no beds of limestone, or quartzite, or iron ore, or graphite. The mass of the formation is eminently granite, with gneissic foliation, which in some cases is well defined, and in others vague or almost absent. What is the relation of the Ottawa gneiss to the Grenville series? The former would be recognized by any petrographer as a granite—a plutonic igneous rock. Professor Adams recognizes the geological identity of the Ottawa gneiss with the Laurentian gneiss and granite which the writer has described as invading the upper division of the Archæan complex (Ontarian system) in the region northwest of Lake Superior. There the igneous irruptive and batholitic character of the granites and gneisses (= Ottawa gneiss) and its invasion of the Upper Archæan rocks is unequivocally demonstrated by evidence which has been abundantly adduced elsewhere. Does the Ottawa gneiss of the Ottawa valley bear a similar batholitic and intrusive relation to the Grenville series? From what the writer knows of the region, it seems to him eminently probable that such will be found to be the case. This hypothesis is favored somewhat by certain harmonious analogies which it would establish between the Archæan complex in the Lake Superior region and the region of the Lower Ottawa. Generally, the Archæan complex throughout Canada, omitting the Norian, is composed of two great divisions. The lower division seems generally to have the petro-

graphical characters of the Ottawa gneiss. The upper division is usually recognizable as an assemblage of metamorphic, sedimentary, or mixed sedimentary and volcanic strata. Part of this upper division has usually been referred to as Huronian, but, according to several authorities, this term was originally applied to a post-Archæan series on the north shore of Lake Huron; and there is some confusion attending its use. Even when applied to Archæan rocks, the term has embraced only a portion of the upper division of the complex. In western Ontario, this upper division includes at least one other group besides that which has usually been called Huronian. The writer has elsewhere proposed the term "Ontarian system," as a comprehensive designation to embrace the whole of the upper division of the Archæan in western Ontario. Now it seems to the writer that the Grenville series in Quebec occupies the same stratigraphical position in the Archæan complex as does the Ontarian system (embracing Contchiching and Keewatin [Huronian?]) in western Ontario. Admitting, for the sake of clearly stating the hypothesis, that the Grenville series is the equivalent of Ontarian system, or any part of it, we would have the following parallelism:—

	Western Ontario and Minnesota.	Eastern Ontario.	Quebec.
In order of super- position.	Ontarian system	Hastings series	Grenville series
	Laurentian system	Ottawa gneiss	Ottawa gneiss
	¹ Car- ltonian { Anortho- sites of Minnesota		Norian
	¹ Car- ltonian { Anortho- sites of Minnesota		Norian
In order of chronological se- quence; an irruptive rock being of later age than the formations which it in- vades.	Laurentian System { Batholitic Granites and Gneisses	Ottawa gneiss	Ottawa gneiss
	Ontarian System	Hastings series	Grenville series

¹ See Bulletin, No. 8, Geolog. and Nat. Hist. Surv. of Minnesota.

If this hypothetical correlation should ever be established, it would then seem that different names and different stratigraphical positions had been given to groups of strata geologically equivalent because of their petrographical dissimilarity. The Grenville series is characterized by limestones and quartzites, with little or no volcanic admixture. In the Ontarian system of western Ontario sedimentary rocks, in a more or less metamorphic state, are common enough; but there is a scarcity of crystalline limestones and quartzites, and altered forms of volcanic rocks abound. This petrographical dissimilarity, however, in no way militates against their geological correlation. It is interesting to note in this connection that the Hastings series, which is geographically between the Quebec region and the Lake Superior region, is intermediate in petrographical character between its suggested equivalents on either side. By some authors it has been correlated with the Grenville series, and by others with the Huronian (Archæan).

GEOLOGY OF TUCUMCARI, NEW MEXICO.

BY W. F. CUMMINS, TEXAS GEOLOGICAL SURVEY, AUSTIN, TEXAS.

IN 1852, Professor Jules Marcou, as United States geologist, made a trip across the country with the engineers who were sent out to survey a railroad route from Fort Smith, Ark., to the Pacific Ocean, near the thirty-second parallel. On that trip he passed through the Tucumcari region, and published a description and section of Pyramid Mountain, one of the representativ

buttes. From the strata at Pyramid Mountain and other places in the vicinity he collected a number of invertebrate fossils, two of which he figured and described as *Ostrea marshii* and *Gryphæa dilatata*, var. Tucumcari, Marcou, and referred them to the same species as those described in Europe under the same names.

The collections of Professor Marcou were placed, by order of the Secretary of War, in the hands of W. P. Blake for description. He differed entirely with Professor Marcou in his identification of the fossils, and referred them to *Ostrea subovata*, Shumard, and *Gryphæa pitcheri*, Morton. Others engaged in the controversy, which became very personal and bitter, and the wounds made by the lances of the combatants have not healed to this day.

The locality was not visited again by any geologist until, in 1888, Mr. R. T. Hill visited the place and made a short stay. He visited it again in 1891 and made further observations. In 1891 I visited the locality, made many sections of the hills in the vicinity, and collected a large number of fossils. This comprises all the geological work in that immediate vicinity, so far as I am informed.

Professor Marcou referred the strata to the Triassic and Jurassic, basing his conclusions as to the Jurassic upon the fossils found there, which he claimed were identical with those found only in the Jurassic of Europe. There is now no dispute about the correctness of his reference of the lower beds to the Triassic.

After Mr. R. T. Hill's first visit he published a paper, in which he said he was inclined to confirm Marcou's reference of the upper beds to the Jurassic. After his second visit he again confirmed Marcou's reference; but upon further consideration he concluded that the beds belonged to the Cretaceous.

When I visited the place I took time to collect fossils and study the stratigraphy and lithological character of the several parts of the formation, and the result was that I could not agree with either of my predecessors. I found evidence there of the existence of the Triassic, Cretaceous, and Tertiary. Since the publication of Mr. Hill's latest paper there is no disagreement between us. Professor Marcou still contends for the correctness of his reference of the upper beds to the Jurassic.

The evidence of the Cretaceous age of the middle part of the strata is based upon the fossils I found in the beds, associated with those from the same place found by Marcou. The following is a list of the fossils found by me:—

Gryphæa dilatata, var. Tucumcari Marcou; *Ostrea marshii*, as determined by Marcou, but in reality *Ostrea subovata*, Shumard; *Gryphæa pitcheri*, Morton; *Exogyra texana*, Roemer; *Ostrea quadruplicata*, Shumard; *Trigonia emoryi*, Con.; *Cardium hillanum*, Sow.; *Cytheria leonensis*, Con.; and a single leaf of a dycotyledonous plant, which I described and figured under the name *Sterculia Drakei*.

It will be apparent to everyone acquainted with the fossils of the Cretaceous that those enumerated belong only to Cretaceous strata, and, if taken from the beds of the Tucumcari region and correctly determined, the conclusion that the beds are Cretaceous would be inevitable.

Professor Marcou, therefore, seeing this, in reviewing my publication, endeavors to avoid the conclusion by saying that either the determinations of the fossils found by me were incorrect or that they did not come from that locality, and suggests that the labels on my packages were loosely put on and became mixed with collections made elsewhere; and on this flimsy subterfuge (to give it no harder name) still insists on the correctness of his reference to the Jurassic.

A simple recital of the circumstances attending the collection, shipment, and determination of the fossils under consideration will be sufficient to satisfy any reasonable mind on both doubted points, especially in the absence of any motive for deception on my part. The facts are that for more than a month prior to the collection of the fossils in dispute we had not collected a single one from any Cretaceous bed, and every package previously collected had been shipped to the museum. Those collected at Tucumcari were shipped overland to Las Vegas, New Mexico, were delayed there for several months, and did not arrive at Austin until every package collected from other localities had been opened and put in the cases. When the boxes containing

the Tucumcari collections arrived, instead of opening them as the other collections were in the storage room, I had them taken to my private room, opened them myself, and put them in a separate case, where they are now with the labels originally placed on them in the field. There were at least fifty packages of these fossils, and each package had two labels attached, so that it is utterly impossible for them to have become mixed by accident or carelessness.

Again, myself and my assistants discussed the fossils in the field as we picked them up, and our note-books show that we then determined them as they are now designated. There can therefore be no reason for supposing that the fossils did not come from that locality, notwithstanding Professor Marcou says that he did not find such fossils there, as if that fact could justify him in saying another explorer did not. Marcou travelled rapidly through the country, made a section at one place, and devoted six hours to the examination of the strata at that precise locality, while I travelled at my leisure, and took all the time necessary to collect the fossils.

He says he has seen the collection of Professor A. Hyatt made in that vicinity, and that there are none of the fossils enumerated by me in his collection. Professor Hyatt has never said that he collected fossils from that locality, and so far as I know he never did; but even if he had, would that be a reason for concluding that another person could not find other fossils? Professor Hyatt has written no paper on that region.

As to the correctness of the determination of the fossils, I took every precaution to prevent any mistake in this matter. I did not wholly rely upon my own judgment, but, after opening up my collection, I made up small suits and sent them to various parties for determination, without giving them the location from which they were collected, but simply asking for specific determinations, and without repeating what others had said, or even giving my own conclusions, and there was unanimous agreement as to all the species I have published.

It will thus be seen that I have taken extraordinary care to be certain of my facts before publishing them and my conclusions drawn therefrom.

The evidence of the Jurassic age of the beds relied upon by Professor Marcou is based upon two species found in the beds, described by him as heretofore mentioned. One of them he calls *Gryphæa dilatata*, var. Tucumcari Marcou, and the other *Ostrea marshii*.

After making my collection at Tucumcari, I sent to Europe for samples of the *Gryphæa dilatata* from the type localities and compared them with Marcou's variety collected by me. The best that can be said is that it may be a variety of the original type.

The samples collected by me of what I suppose was his *Ostrea marshii* are not *O. marshii*, but *O. subovata* of Shumard. We have hundreds of specimens of *O. subovata* in the museum, collected from well-known Cretaceous horizons, and upon comparison with them the specimens from Tucumcari are found to agree in every essential particular. Therefore the proof of the Jurassic age of the beds is narrowed down to one fossil, and that only a variety of the form found in the Jurassic of Europe, and which has not been reported from any of the well-known Jurassic horizons in North America. This will certainly not be considered sufficient to establish the Jurassic age of the beds when there is associated with it the other forms enumerated which are certainly Cretaceous.

I placed a great deal of stress upon the fact of having found in these beds a dicotyledonous leaf, as proving the Cretaceous age of the beds, for the reason that, so far as I know, no dicotyledons have been reported from any strata lower than the Cretaceous, in either North America or Europe. It is true that they have been reported from beds which some geologists held to be Jurassic, but which by others were referred to the Cretaceous upon the very ground that they contained dicotyledons.

Professor A. Hyatt has been quoted as expressing the opinion in private that the beds at Tucumcari were Jurassic, but in a recent article he deprecates such a use of his opinions privately expressed, says it was unauthorized, and asserts that he has no opinion on the subject.

THE MARINE BIOLOGICAL STATION OF THE UNIVERSITY OF TEXAS.

BY CHARLES L. EDWARDS, AUSTIN, TEXAS.

ANY ONE familiar with the brilliant pioneer work of Agassiz, Pourtalès, and Brooks, together with their students, upon the fauna of the Gulf of Mexico and neighboring waters will gladly welcome the inauguration of any movement, however modest, to continue the exploration of the American Mediterranean. The successive summer expeditions sent from the Johns Hopkins University since 1887, to the different Bahama Islands and Jamaica, and the proposed establishment of the Columbus Station on the latter islands, are familiar to all interested in the work of this field.

An important part of the School of Biology, created in January, 1892, by the regents of the University of Texas, both as regards instruction and research, is the Gulf of Mexico Station. As a result of a brief preliminary survey which, at the request of the regents, I made last summer at Aransas Pass and Galveston, several facts of prime importance in locating a biological station became apparent. The low Texas coast is bordered by exceedingly shallow bays, from two to ten miles wide, cut off from the Gulf of Mexico by a very narrow sand-formation. This almost continuous stretch of sand, raised unevenly by innumerable dunes formed by the wind, is broken at eight places by narrow channels into seven islands, and at three other points partially unites with the mainland to form extended peninsulas. Its gulf shore is unindented, while, on the other hand, its bay shore-line is quite irregular. Vast areas of the bays are exposed at low tide, forming mud-flats; while even in Corpus Christi and Matagorda Bays the depth does not exceed fifteen feet. Since the mean tide is less than half a foot at most places in the bays, an advantageous location for a biological station must be contiguous to the free waters of the Gulf. Even then one must go from along most of the outer shore five miles to seaward, in order to reach the ten-fathom curve. Directly off the entrance to Galveston Bay this depth of less than ten fathoms extends for a distance of thirty miles.

Reaching from near the mouth of the Rio Grande along the extreme southern Texas coast for one hundred and fifty miles northward is Padre Island. The bay which it cuts off, Laguna Madre, is for the most part a vast mud flat, and the Padre itself is inaccessible. Farther north, at either Aransas Pass, where Corpus Christi and Aransas Bays empty into the Gulf, or at Pass Cavallo, the entrance to Matagorda Bay, would be, with a suitable building, an excellent location for the station. The entrance to Galveston Bay, while in some respects not having the natural advantages of the other two locations, yet is much more accessible. Here is a highly desirable building, which cost some \$15,000, soon to be vacated by the Quarantine Department. Since this building belongs to the State, the Galveston location was recommended and a bill was introduced in the 23d legislature of Texas to set aside the present officers' quarters of the Quarantine Department at Galveston for the purposes of the Marine Biological Station of the University of Texas. With a further item of \$5,000 for equipment, the bill was favorably reported from the Committee on Grounds and Buildings of the House, but, owing to the large number of measures having precedence, this bill, unfortunately, was not considered.

Besides a building fully supplied with the necessary aquaria, microscopes, reagents, etc., for laboratory study, and boats of light draught for work in the shallow water, it is planned, after the idea of Dohrn¹ for the Naples Station, to equip a seaworthy steamer as a floating station for deep-sea collection and observation in the waters of the Gulf of Mexico. For the wonderful possibilities of this field in addition to the eloquent testimony of A. Agassiz,² in his description of the work on the "Blake" expeditions, need be added.

As Professor Whitman has so ably demonstrated in building up a national station for marine biology at Woods Holl, it is in generous coöperation that the science can best be advanced. The University of Texas extends a welcome to any investigator in the various lines of biology who may desire access to the fauna and flora of the Gulf of Mexico. Going from the various stations established by Agassiz, Brooks, Whitman, and others on the Atlantic and Gulf coasts, to that of Jordan on the Pacific, the investigator may have the enviable advantage of studying a special group of animals or plants under the most diverse geographical conditions. Once this migration, which in Germany is so enlarging and helpful to the student, is made possible among our biological stations, the great advantages are too apparent to need mention.

Since the best conditions are not this summer available for work on the Texas coast, it has been decided to hold the first session of the marine station in the Bemini Islands, Bahamas. Applications for admission will be received until June 20. Of investigators no special fee is required. For students not attending the University of Texas, there is a laboratory fee of \$25. The necessary expenses for the session, including transportation from Austin and return, will approximate \$100.

METHOD OF MAKING A SANITARY INVESTIGATION OF A RIVER.

BY CHAS. C. BROWN, C. E., UNION COLLEGE, SCHENECTADY, N. Y.

THE following programme of the investigation of the Hudson River and its tributaries has been worked out in our labors for the State Board of Health during the last four years, and may be of interest. The work commenced with an inspection of the shores of the river to determine the causes of certain nuisances which existed along its banks and to determine the method of abating these nuisances. These consisted principally of marshes or badly drained pools with some areas partly covered with water at high stages but open to the sun at low water. A few nuisances arising from the deposit of garbage or the discharge of sewage were also found. All these were evident on inspection and it was possible to abate most of them with little difficulty. In any other case the same would usually be true, except where there are large areas of bottom land which are overflowed by high water and are not well situated for drainage after the high stage is passed. It is possible, however, except in the largest watersheds, to drain much the largest part of such lands. There is no question that such bad conditions as are often found have a decided effect upon the salubrity of the neighboring lands, and that sooner or later treatment of the problem of drainage must begin and be carried through as rapidly as funds will permit.

Where a river is also used as a source of water supply, a much more detailed study must be made of its condition and possibilities. In the case of the Hudson, a study of the geology of the watershed was made to show what the inorganic chemical impurities of the water might be, the result being very favorable to the purity of the water, as much of the area is covered by the oldest formations and supplies but little inorganic matter of any sort. The southern tributaries of the Mohawk bring in more such matter, in the form of lime from the Helderberg and neighboring formations, than any other part of the watershed. Where the surface soil is made up of disintegrated rock it may have a beneficial effect upon the water by acting as a filter to remove some of the organic matter, or it may have a deleterious effect by adding much alkaline matter to water percolating through it. In some cases this may be so serious as to prevent the use of water from some parts of the watershed for water supply. In connection with this study of the geology goes the study of the organic pollution from vegetable sources, since much of this comes from marshes and swamps whose existence is due to the arrangement of the geological strata. There are cases where the amount of such pollution is excessive. It is probable, however, that there are very few cases where the swamps cannot be drained and thus

¹ Bericht über die Zoologische Station während der Jahre 1885-1892. Mittheil. Zool. Station, Neapel, 10 Bd., 1893, pp. 633-674.

² Three Cruises of the "Blake," Boston, 1888.

greatly improve the condition of the water. Much of the water from the upper Hudson and the northern tributaries of the Mohawk shows the effect of this sort of pollution, but the dilution from comparatively unpolluted sources is great enough to reduce it far below the objectionable point.

After this study, which usually is not very important, comes the study of the pollution by the population on the watershed. In investigating the purity of an established supply or in selecting a new one this is the most important study of all. We have approached the subject in three ways, keeping the results obtained by the three processes in juxtaposition so that they can be used as mutually explanatory. The first and, after sufficient experience in judging effects, the best method is by actual inspection of the polluting matters discharged into the stream. These matters consist of sewage, garbage, drainage from fertilized fields and other sources of animal matter, and the discharges from manufacturing establishments, some of which are chemicals and some putrescible organic refuse. In connection with this inspection goes a determination of the amount of water flowing in the stream at the point of entrance of the polluting matter. A careful study of the relative amounts of polluted liquid and river water with the proper consideration of the amount of pollution the water already contains and the character of the new supply will give a very clear idea of the condition of the resulting mixture. Detailed study by chemical and biological methods of typical conditions will bring one to the ability to determine by the inspection the probable condition of new streams in approximately the same circumstances. In our own case, having but little material obtained under the conditions existing in this country, it was necessary to make this detailed examination.

The second process applied to the river was, therefore, the chemical analysis of numerous samples from various places. It is usually considered that the elements determined in water analysis which denote the amounts of organic matter in different forms and the amount of salt are the important ones. The list included, therefore, albuminoid and free ammonia, nitrogen as nitrates and as nitrites, chlorine, oxygen absorbed, as well as the total solids, loss on ignition, color, appearance, and odor at 100° F.

The third process applied was the biological analysis of samples of water taken from the same places as those for chemical analysis and from many more. The biological analysis may be made with reference to the number and kinds of algæ, infusoria, and other microscopic animal and vegetable life, with reference to the numbers of bacteria, and with reference to the numbers of such species of bacteria as can be recognized as coming from sources which are dangerous or suspicious. All of these determinations will be valuable in deriving a basis for a definite opinion as to the character of the water. A few experiments were made under the first head, and it was found that as regards flowing water in rivers this determination was not as necessary as others, moreover, the methods of such analysis have been well elaborated by others, therefore it was left until a more convenient season. It is advisable to make this analysis on some samples, however, to secure the fullest knowledge possible.

As regards the determination of numbers of bacteria, it was soon discovered that in order to determine the relative amounts of pollution in the rivers at different places, it is necessary to reduce the possible sources of addition to the numbers of bacteria to a minimum. This is done by waiting until a rainless period, or nearly so, has intervened of sufficient duration to reduce the river to its low stage and then give time for the collection of the samples. It is possible by taking this precaution to show the increase in pollution by showing the increase in numbers of bacteria below the source of pollution. Rains, sharp and heavy or long continued, will wash into the stream much matter from the soil, abounding in bacteria, so that the indications of pollution from the constant flow of sewers and the like will be greatly obscured. It is probable, also, that it would be difficult to compare the purity of two streams in widely different parts of the country or in different conditions as to soil, slopes, area of watershed, etc., on account of the consequent variations in numbers from

what may be called outside sources. Our work has shown the possibility of determining the relative increase in pollution in the flow of a single river when proper precautions are taken. One point to be mentioned is that where sewage is discharged into still water much of it will settle at once and samples taken near the surface will not show the full effect of its presence.

The numbers of bacteria, at least unless determined without the greatest care to eliminate all disturbing conditions, are not therefore so important an indication as is desired. It is well known that there are species of bacteria which exist under their most favorable conditions in the intestines of men and the higher animals, but will live for a greater or less length of time in other places. If the numbers of such bacteria could be ascertained the relative amount of pollution from such suspicious sources would be best determined. On the recommendation of Dr. Theobald Smith of Washington we tried the method of determining the numbers of *Bacillus coli communis*, by the fermentation-tube method. This bacillus was selected because it is the most common in fecal matter and its growth in such media as ordinary river water at ordinary temperatures is believed to be very slow, if there is any. The method suggested by Dr. Smith was found to be easily applicable in practice, and some very valuable results have been secured. It promises to be a most valuable aid in determining the pollution of water, and is especially valuable because it gives the statement of the serious part of the pollution, while all the other methods mentioned are open to the objection that they may reject a water which has a large amount of pollution not from dangerous sources, the water being, therefore, comparatively clean; while they may, on the other hand, pass as good water which shows but little actual amount, that amount being of a very suspicious nature.

Many of the results of the investigation whose programme is presented above, are given in the last four reports (tenth to thirteenth) of the N. Y. State Board of Health. A paper by Dr. Smith, giving the argument for the selection of *B. coli communis* as the index of pollution, is given in the thirteenth report. The investigation is not yet finished, and other reports will follow until sufficient data have been obtained to warrant a definite statement as to the condition of the water at the intakes of the various waterworks which draw from the lower parts of the rivers under investigation. It is believed that the work done shows that the programme given above is the proper one to follow, and this statement of it is therefore made with the hope that it will be found useful in other cases. Considerable experience in selecting conditions and in collecting samples and data will be necessary to make one expert in drawing definite conclusions from the results obtained.

NOTES AND NEWS.

THE museums and laboratories of the University of Pennsylvania are represented by a very considerable exhibit at the Columbian Exposition. The University has erected an inclosure on the space assigned to it in the gallery of the Liberal Arts Building, for which the design was contributed by the University School of Architecture. This forms the exhibit of that school. Within this space are contained various collections from the different departments. Notably the Veterinary and Biological. The latter contributes a psychological laboratory, which will be in operation during the summer. The Department of Archæology has sent collections from three of its sections, American, Babylonian, and Egyptian, which are now installed in the gallery. The large collection of games and religious objects contained in the Oriental Section of the Museum will be shown in the Anthropological Building.

—The present interest in subjects connected with the study of sociology has led Professor Charles R. Henderson of the University of Chicago to prepare "An Introduction to the Study of the Dependent, Defective, and Delinquent Classes," which will be published about June 1 by D. C. Heath & Co., Boston and Chicago.

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Attention is called to the "Wants" column. It is invaluable to those who use it in soliciting information or seeking new positions. The name and address of applicants should be given in full, so that answers will go direct to them. The "Exchange" column is likewise open.

SIGN LANGUAGE IN PRINT.

BY FREDERICK STARR, UNIVERSITY OF CHICAGO, CHICAGO, ILL.

MY attention has lately been called to a matter which seems to me of some interest. It is well known to all readers of *Science* that gesture language is a common means of communication between our different Indian tribes.

Mr. Lewis Hadley of Chicago is at present engaged upon a plan for reducing the sign language to print. The purpose of the work is benevolent and religious, the idea being to bring religious instruction to the old Indians. It is well known that old Indians will never learn to read our language. It is believed by Mr. Hadley and his friends that they will quickly learn a printed sign language. Of course, all these old men make constant use of gestures and signs; and, if they take kindly to the printed gestures, there is no question that considerable progress might be made.

Mr. Hadley has had difficulties to contend with in carrying out his work. He has been hampered by the lack of funds and by the novelty of the undertaking. In his first experiment he cut the dies for printing himself, and the resulting impressions were black designs with the figures in white lines, and the result was exceedingly ugly. He has since then simplified the designs and made them in the form of ordinary type, and has now an extensive font of several thousand types, which will be used in printing cards and tracts for the instruction of the Indians.

There are two points to be considered in reference to this plan: First, its feasibility; second, its methodology.

There are three questions that arise in reference to feasibility:—

1. Is there a universal sign language among the Indians?
2. Can the signs be represented by type?
3. Will the Indians care to learn it?

1. As regards the first of these, Mallery says that there is no absolutely fixed sign language in general use among the Indians. While this is true, it is also true that all Indians gesture, and the gestures are so natural and so self-expressive that there is no question that natural signs, although new, would be generally understood.

2. There is, of course, a difficulty in representing the gestures by type so that they can be readily recognized. This difficulty all who have attempted to work in the subject of gesture language realize. Mr. Hadley has changed the forms of his type repeatedly. He has produced finally what appears to be simple, plain, and easily understood characters. Many of these may have to be still further changed, but in large measure they meet the requirements.

3. There is a very serious question as to the favorable reception of this printed gesture language by the Indians themselves. It is, however, in a certain sense picture-writing, and picture-writing is natural to the North American. Mr. Hadley is doing, on a large scale and at one stroke, what the Indians have begun to do in many cases. Colonel Mallery has shown in his papers the

close relationship between gesture language and pictography. The picture character is often only an attempt to represent a gesture. This being so, it may be possible that a kind reception will be given by the Indians to the printed sign language.

As to the method of introducing the printed sign language into use, Mr. Hadley has devised a game of cards, which, he believes, will help greatly in the work of teaching. Each card has upon its face, in unusually large type, a gesture. Upon its back is printed the English equivalent for the gesture. The game to be played with these cards is based upon certain gambling games, already quite familiar to the Indian, and success in the game depends upon the Indian giving the English word for the sign represented. All games of an instructive kind are more or less of a nuisance, but it is not impossible that these cards may be successful in the way they are intended. Besides the game of cards, the purpose of which is really to teach the speaking and reading of English through the printed sign language, a considerable number of texts, mainly of a religious character, are to be issued. It is expected that an Indian who has a story or a passage printed in the sign character will himself make the signs represented, and by making the signs he will gain the idea to be conveyed.

Every text of the sign type has under it the English equivalent words. In order to convey an idea what this test is like, I present herewith a line of the text as it appears in print.



It will be seen that, quite apart from its religious and educational purpose, this matter is one of scientific importance, and we shall watch with interest how far it may succeed.

CURRENT NOTES ON ANTHROPOLOGY.—XXVIII.

[Edited by D. G. Brinton, M.D., LL.D., D.Sc.]

The Present Position of the Hittite Question.

THE ethnic position of the Hittites has been a perplexing question for many years. It seems to have been answered in a certain degree by the recent excavations of Von Luschan at Sindjirli, which is in ancient Hittite territory. Halévy has shown that the two stelae brought from there to Berlin are in a Phoenician dialect. The Hittites of the Bible were, therefore, probably Semites.

Unfortunately, this solution leaves the real problem untouched; for it is now clearly established by Belck, Lehmann, and others, that the mysterious syllabic inscriptions and bas-reliefs at Pteris, Nymphi, and about Lake Van, were not by the Semitic Hittites, but by a wholly different people, who called themselves Chaldi, after their chief divinity, Chaldis. To their land they gave the name Biaina (Urartu, —Ararat, in Assyrian), and their chief city Van, their own name of which was Tuspa-na, was founded about 833, B.C., by their early king, Menuas.

The confusion partly arose from the fact that the Semitic Hittites, previously tributary to the Assyrian monarchs, were subjugated by the Chaldi king, Argistis I., about 800, B.C.; and, further, that at the fall of the Chaldic kingdom, about the close of the seventh century, B.C., many of the Chaldic people were driven southward into Cilicia and its neighborhood.

The question therefore remains, Who were the Chaldi? The prevailing theory has been that their language had Mongolian or Turkish affinities; but Professor Sayce has pretty clearly shown that it had regular declensions, a nominative ending in *s*, an accusative in *n*, oblique cases in terminal vowels, and an adjective which followed the noun and agreed with it in these respects. This is not at all analogous to any Mongolian or Turanian language, and, if correct, disproves the theory.

A bolder one is advanced, not entirely for the first time, by M. Salomon Reinach in the *Revue Archéologique* for January. He maintains that the migration of the Chaldi, or supposed Hittites,

was from west to east; that they were emigrants from European lands; and, in fact, were none other than a part of our old friends, the Pelasgi of Greece; and these in turn were of the same stock as the Tyrseni and Etrusci of Italy. Sufficient can be said for the theory to make it worth further and serious investigation.

An older theory, to wit, that the true Hittite is an Indo-European language most akin to the modern Armenian and probably its ancestor, has been revived with considerable force by Professor Peter Jensen of the University of Marburg. His article is in the *Sunday-School Times*, March 25 and April 1. His criticism on Peiser's theory, that it is allied to the Turkish, is severe and merited. With reference to the puzzling and complex questions suggested by the inscriptions and ethnic types presented by the ancient monuments of Cilicia, he proposes the theory that the rulers of this district were at one time Semites or strongly semitized, while the mass of the population was of Indo-European blood. His opinion of his predecessors' studies is briefly summed up in these words, with reference to those of Sayce, Conder, Peiser, Ball, and Wright: "All are without foundation, and their results are destitute of value"!

A Linguistic Map of Guatemala.

Dr. Karl Sapper of Coban, Guatemala, has published in the first number of *Petermann's Mittheilungen* for the current year a map showing the present distribution of the native languages in Guatemala, accompanying it with a carefully prepared article on the dialects and culture conditions of the descendants of the aborigenes of that country. Much of it is from his own studies, much of it from the excellent works of Dr. Stoll. He does not seem to be aware of the publication by me of the material collected by Dr. Berendt on the Xinca, the Pipil of Acasaguastlan, and other dialects. He falls into the rather serious error, which I pointed out in a paper published by the Congress of Americanists (session of 1890), of locating a language of the Mixe group in Guatemala, though he adds that no signs of it now exist. It never was there. He fails to solve the only real obscurity which remains in the linguistics of Guatemala, that is, the identification of the Popolucan located by the historian Juarros at Conguaco, in the partido of Guazacapan, which was *not* Xinca.

The language of Yupiltepec he considers a dialect of the Zinca, and brings into closer relationship the Chorti and the Chol. His expressions about the dialect of the Cajaboneros are not clear; in one sentence he speaks of their tongue as containing elements fundamentally diverse, "ursprünglich fremd," to the Kekchi; and in another refers to this element as perhaps Chol, which is merely another Maya dialect.

While Dr. Sapper's work is open to these slight criticisms, it is in the main worthy of the highest praise.

The Earliest Extension of the Iron Age.

In these notes (*Science*, March 10) I referred to some recent studies on the early Iron Age in central Europe. The question still remains, When and how did the art of working iron reach those localities? Two valuable papers of late publication have interesting suggestions touching this point. One is on "Le Premier Age du Fer au Caucase," by M. Ernest Chantre, who for twenty years has travelled, studied and excavated in the Caucasus; the other by M. Louis Siret, scarcely less distinguished for his archæological campaigns in Spain. Some remarkable coincidences are pointed out by both.

M. Chantre finds that the most ancient sepulchres in Lower Chaldea which contain iron are shown by their funerary contents to be contemporaneous with the third and fourth dynasties of Egypt, at which period occur the first signs of this industry on the Nile. At the lowest, this would place them 2500 years, B.C. The knowledge of the metal reached the southern and central vales of the Caucasus about 1500 B.C., through the extension of a "Semito-Kushite" people, who were the ancestors of the modern Ossetes. They were distinctly non-Aryans, and the art of working iron was not introduced by them into Europe. Later on, about the seventh century, B.C., their culture was deeply

modified by irruptions of Mongolic hordes from the East. (All this in spite of the fact that the modern Ossetes speak an Aryan tongue!)

The proof of this early Semitic influence is found in the identity of art-motives, decorations and methods, and especially in the numerous traces of the worship of the goddess Ishtar, the Astarte of the Phœnicians. In the Caucasus, as elsewhere, her favorite symbol, the dove, is constantly met with in ancient tombs; as is also that of the hand, employed in her rites as the symbol of adoration and peace.

It is true, as M. Chantre remarks, that in every station of the earliest iron age in Europe, from Greece to Scandinavia, we find figurines of birds, evidently sacred, and all to be traced to the dove of Astarte. They are proofs of what impressed M. Siret so much in his study of the earliest civilization of the Iberian Peninsula,—"the worship of a female deity represented under various symbols." He also, in his article in *L'Anthropologie*, 1892, No. 4, is forced by the results of his own excavations to assign this civilization to the daring early navigators of Semitic blood, to the Phœnicians, sailing from the far east of the Mediterranean, rounding the rocky shores of Spain in search of tin from the Cassiterides, or amber from the far-off shores of the Baltic. The first signs of iron there follow without a break on a highly developed bronze period; and its earliest discovered use was as rivets to fasten together plates of bronze. This indicates peaceable introduction and artistic growth, not the result of violence and conquest. The merchant, not the warrior, was the civilizer.

LETTERS TO THE EDITOR.

. Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.

On request in advance, one hundred copies of the number containing his communication will be furnished free to any correspondent.

The editor will be glad to publish any queries consonant with the character of the journal.

Sham Biology.

THE article "On the Emergence of a Sham Biology in America" undoubtedly brought much joy to many botanists. Some of us know from experience that many American botanists are never so much tickled as when some one has gotten them to believe a zoölogist is hopping about a botanist and is worrying over the adhesive soil the botanist is trying to shake from his own trousers and boots onto those of the zoölogist.

Feeling himself above any and all of the charges made in the interesting tirade, the present writer has concluded he ought to at least make an attempt to show how strong a position his colleague had taken. No chuckling botanist can have any rational ground for gleefully pointing to me as a zoölogist badly hurt. Notwithstanding this, I am fully aware of the fact that nothing others may do or say is too minute to impel some people to strike their breasts, pour out eloquent prayers of thanks, and then go their way rejoicing over the capital they imagine can be made out of the sins of others. I also must say I am not at all sure of what my fellow-zoölogists will think of me for daring to answer for others. The unqualified and sweeping statements in several places technically include me, and this fact I offer as an excuse for attempting to indicate to botanists that the "sham biology" article is not so impregnable a piece of scientific work as I know many think it is.

It may be well to forestall possible taunting thrusts by stating that I have never desired to give a course in "general biology;" that I never attempted to plan, nor even thought of planning, a course in general biology to be given under my supervision, though the opportunity to do so was before me when I came to the University of Minnesota as an instructor. I have always insisted, and now insist, upon the independence, the autonomy of the two sub-departments of biology. My whole record stands as a proof of this, and therefore it cannot be said with justice that I belong to a class often called "sore-heads."

Let us at the outset agree to take the figures of speech for what they evidently were intended to illustrate, and not try to divert the real issues by seizing an opportunity to nag our immaculate brethren in botany.

We can all agree to the definition of biology. If it is to be improved at all, it must be made more general by saying it is the science of matter in the living condition. The writer tells us the word is "still to be defined as by Lamarck and Treviranus—both distinguished botanists." Will the botanists observe the innocent way in which their extremely generous champion here puts forth a foreclosed first mortgage claim on these distinguished naturalists—*biologists*. And this, too, so imperturbably in a discussion whose whole tenor is to brand good zoologists as out-and-out usurpers. Now couple the definition of biology and this innocent act with "I have not at present time to discuss the fundamental absurdity of courses in 'general biology'—as if it were possible to plunge boldly into comparative study of plants and animals before one has studied plants and animals themselves." Who is now sure of what the writer means by "biology" and "general biology?" Is general biology a bold plunge into the comparative study of plants and animals before one has studied plants and animals themselves? Perhaps it is. Perhaps it is not. I have yet to learn of the fool board of trustees that is paying some fool instructor attempting to compare things without considering the things being compared. Is a course in "general botany" a course in the comparative anatomy of plants? Is a course in "general biology" to be proper only when it brings the student to the level of the genius who has been studying plants and animals for thirty or more years? If it is, then away with your "fundamental absurdity" of long courses and short courses and three-month courses in botany—as if a man could plunge into comparative study of plants without studying plants themselves! The figure of following "analytical statics" "up by geometry and the calculus" is not at all to the point, and it indicates an altogether different conception of the term biology and the phrase "general biology" than one would be led to expect in view of the explanation previously given. It might be well for our iconoclast to consider the definition of biology above suggested, and to ask, What is a plant and what is an animal? I do not mean to imply that in biology we are not to call living things plants or animals and that the student is not to study the things under these names. At present it is impracticable to do otherwise. But I do mean to imply that it is possible to teach the general laws and principles of biology in connection with the study of both plants and animals themselves. Moreover, I maintain that it is beyond rational objection that the student who studies well a *Pteris* and a *Lumbricus* (I use the words to indicate centres) has a broader and better foundation of facts for the great generalizations of biology than the student who studies only a *Pteris* and a *Ranunculus*. We will admit without discussion that the end to be attained by a general course is both informational and disciplinary. Now I cannot admit that the methods of study in botany are inherently different from those of zoology. Botany and zoology are coördinates of biology, and the methods in either must be biological methods. So far as the discipline is concerned, therefore, a term devoted to the study of plants and one to the study of animals will give as good results as two terms devoted to the study of plants, and certainly more information of value in every-day life will be gained in the former course. I cannot see why this could not be true even if sixteen full weeks, for example, were given up to the study of animals and only eight to plants. (The botanists have my permission to exchange the number of weeks.)

After all, it is not evident to me that the "fundamental absurdity" has caused this spasmodic cough of the botanist. The thorn seems rather to be—some zoologists are conducting courses in "general biology," and, naturally enough, ultimately guide the more interested students into zoological lines of work. The botanists have confessed that they can't conduct such courses because they become so one-sided as to be unable to see that there is anything good in zoology they can't duplicate in botany—they become, if I may venture a figure, soles with their eyes on the under side, and then they croak about "fundamental absurdities." Is jealousy loose among our botanists? It is safe to say if the botanists had this monopoly of courses in general biology they would not talk about "fundamental absurdities."

It seems to me that the paragraph containing the words "funda-

mental absurdity" is full of extravagant utterance. Is it not absurd to say a zoologist can't write anything worth reading upon the anatomy of *Pteris*? Is it not absurd to say only specialists can write anything of value? Either such statements are wild and not sufficiently guarded or the botanists are all fools. And even fools sometimes write things worth quoting. Certainly the zoologists do not at present believe the work of all botanists is unreliable and should never be referred to.

I now come to the specific charges. Without giving any reasons, I must beg to differ entirely with the writer as to the question of the phylogeny and ontogeny of the "sham biology."

The matter of the inadvertent use of the term biology is, of course, to be regretted. But it ought to mollify the iconoclast somewhat to see that the nomenclature in our university organization is nowhere logical and consistent throughout.

I am not sure that I know just what is meant by saying Johns Hopkins University would have a better influence with "an honest naming of the zoological courses that *were* provided for." I was not aware they had ever been named otherwise. Certainly the zoological courses were named in accordance with their subject matter when I was at Johns Hopkins University. And I can assure the writer it will take more than brilliant rhetoric and insinuations to make me believe conditions have so changed as to have necessitated a dishonest naming of the zoological courses. I cannot speak positively about the present course in general biology at this noble university, and will therefore speak only of what existed several years ago. The course in general biology, as I got it, was by no means as one-sided as any botanist would have made it. A general biology course that will develop enough love for botany in a student to make the purchase of such expensive books as Sachs and various monographs a pleasure, and that will develop a respect for botany and admiration of botanists and their magnificent work, such as I know has been developed at Johns Hopkins, should hardly be dubbed "sham biology," simply because zoologists were in power. If the facts gotten were wrong, it was the botanist's own fault; for the references were to the recognized authorities. If the course leaned any way in my case, it was toward botany. I have heard it said the course took up two or three plants and a dozen or more animals—that a few weeks were given to plants and months to animals. That certainly is not a true picture of the course I was privileged to get. And why must a course, in order to be above the shadow of a "sham biology," consider just as many plants as animals? Must just as much time be devoted to plants as to animals? It seems to me no weaker principle could be adopted. If the botanist directs such a course of study, he will naturally illustrate more with plants. The zoologist will naturally use animals more frequently. But this does not necessarily produce a sham biology. I admit a decided zoologist or a decided botanist will always be in danger of curtailing the sister-science too much, and a course laid out by the one or the other may, naturally enough, not be altogether satisfactory to the colleague "not in it." Such facts do not touch the possibility of botanist and zoologist conjointly formulating a course in general biology. If the principles, laws, and generalizations to be impressed upon the student be taken as the guide, and the two kingdoms of living things be viewed as the store-houses of facts, a true general biology becomes a possibility. Why should a school of biology, organized with a professor of zoology at its head be any more a school of sham biology than a university with an ichthyologist as president be a sham university?

The "always insular capabilities of the Johns Hopkins biologist for blatant philistinism in regard to things botanical" would be an unpardonable fling in view of what men the writer's previous statements would make it cover were it not for the fact that it appears to have been written with the ghost of that pamphlet (which I had supposed dead, because of its absolute flatness) dancing before him in "cool effrontery." But even the pachydermatous zoologists can appreciate moderation; and it is no weakness to keep one's just appreciation of an evil that does exist in some places under the influence of reason.

There are many things in the way of criticism and explanation yet to be said, but I will close by pointing out what an influence

botany seems to be having on my estimable colleague. Some zoölogists divide the organs of animals into the vegetative organs, their functions being those common to plants and animals, and the organs of animal function, their functions being characteristic of animals. My genial associate must have learned this fact from some one and makes a desperate effort to use it in classifying the sub-sciences of biology by trying to limit zoölogy to the vegetative organs of animals, and relegating the animal functions to psychology, which is held "coördinate with zoölogy rather than as one of its sub-divisions." I may be wrong, but that effort looks like a bid for a vote.

To guard against any misapprehension on the part of those not acquainted with the actual attitude of the departments of botany and animal biology toward each other at the University of Minnesota, I must say that Professor MacMillan and myself are not at loggerheads here, but that we do and always have pulled together for the equal advancement of both botany and animal biology. The adjustment of our courses is not the result of a compromise, but the individual and united recognition of facts and conditions. We are not competitors, and there is no likelihood that we shall become such.

HENRY F. NACHTRIEB.

Professor of Animal Biology, University of Minnesota.

April 18.

On Methods of Defending the Existence of a Sham Biology in America.

TWO recent papers in *Science* deserve a little attention at this time, for they serve as examples of the kaleidoscopic movements by which "biologists" hope to defend themselves against the clearly stated charges of incorrect use of terminology which have been brought against them. It will not be permitted to these wanderers from the path of orthographic rectitude to conceal their retreat under cover of a sea of ink. The discretion, good taste, enthusiasm of the writer, are not the subjects of the discussion and will not be discussed by him. No shuffling to alien positions can be admitted as an answer to the definite impeachment which has been brought against courses in zoölogy masquerading under the erroneous name of biology.

Although the briefer, the article by Mr. H. F. Osborn¹ of Columbia College should, from the acknowledged ability of its writer and its air of gentlemanly candor, be given first consideration. Mr. Osborn is under such manifest misapprehension, however, that it will be necessary first of all to correct him and indicate to him just the point at issue. He says "the arrangement of courses in Columbia is cited by Mr. MacMillan as a leading example of the manner in which botany is subordinated to zoölogy." Since absolutely nothing was said in my article about the subordination of botany to zoölogy at Columbia or anywhere else, I am naturally interested to learn by what higher criticism, textual or literary, Mr. Osborn has arrived at such an unexpected result. In my former paper it is written, "At Columbia College it is apparent that the subject of botany, since it stands by itself under its own organization, is supposed at least by the 'biologists' of that institution to be quite without the pale of their own science." It is my evident and distinct purpose here to charge, not subordination, but misuse of terminology. Indeed, if there were any "subordination" at Columbia, I should think it would be of the zoölogical courses staggering as they are under the weight of a false nomenclature.

In his note, Mr. Osborn cites a number of botanical and zoölogical courses at Columbia and then uses the word "biological" correctly in the sentence, "It does not appear that botany is ignored in this programme of *biological* courses in this institution." Immediately afterward he uses the word incorrectly when he says, "the fact that the botanical courses are not arranged under the *Biological* Department is a mere technicality of administration." A "Biological Department" without botanical courses is, however, something more than a "technicality"; it is a sham. Mr. Osborn is, of course, at liberty to have his department separated as he will; it is no affair of ours,—but why should he permit such a line as this from the circular of information,²

"Biology (Zoölogy) . . . Professor Osborn"? Why does he appear as defining the word biology as zoölogy? I am sure it must be for some better reason than the anxiety to use a high-sounding word, even though that word be used incorrectly.

Having thus indicated to Mr. Osborn the errors into which a probably hasty perusal of my former article has led him, I may now note his principal defensive movement. He says, "Biology, however, is not the science of animals and of plants, as Mr. MacMillan maintains, it is rather the science of life." Therefore, "those who set forth the fundamental principles of life are biologists,"—a fair paraphrase, I trust, of Mr. Osborn's argument. This is so unexpected a point of view to be taken by one of the leading animal morphologists of America that it is indeed difficult to collect one's self for a reply. The venerable style of talk about "life," I supposed, was extinct in scientific circles, unless one includes the metaphysicians. "Life," I had supposed, was an abstraction from certain observed phenomena of a group of things known as plants and animals. I presume Mr. Osborn does not use the word as does the Boston University in its Year Book,³ where Group IX. in Courses of Instruction is "Chemistry, *Biology*, and Geology," and Group X. is "*Life*, Personal Development, and Expression." I did not suppose that "biology was the science of living things" could possibly find objection in such a quarter as Columbia College. Here at Minnesota we are busily studying living things, but if Mr. Osborn is studying "life," he is evidently on another plane altogether. Long ago, one used to hear of "vital force" and "life," but I supposed we now believed that the best way to learn about life was to study living things. If it is true that the zoölogists are going in for the study of "life" under the belief that biology is not the science of living things, I wish them God-speed on a perilous, if ancient, voyage. And if this really is the modern view of "biology," I yield me a captive to Mr. Osborn's convincing argument and beg to withdraw among those botanists who believe that botany is the science of the living things, plants, and will certainly, if I know them, be glad to leave the study of "life" open to the zoölogist—"biologist," who rules out living things as irrelevant to his science.

Let me, in closing, call the attention of Mr. Osborn to the fact that I am unaware of any one-sided state of true biological education in America. There is nothing one-sided about it in Harvard University. It is the sham biology that is one-sided, and for this the zoölogists are responsible in large measure, therefore the epistle is addressed to them. I recall now but one institution which names its botanical courses, a "department of biology." And this department is manned by a Johns Hopkins doctor of philosophy, from whom one might unfortunately expect the one-sided view.

The paper by Mr. Francis H. Herrick,⁴ entitled "On the Teaching of Biology," requires some elucidation and correction that I may venture to give. Notwithstanding its characterization of my former article as "thoroughly bad," I take pleasure in acknowledging its own uncommon excellence. Any defense of the sham biology is sufficiently difficult, and while the air of righteous enthusiasm was accurately enough predicted it was scarcely realized with what vigor the plaintiff's attorney would be afforded the treatment sanctioned in such cases by all the traditions of the bar.

Aside from its entertaining personal character, the contribution by Mr. Herrick appears to seek the establishment of the following points: (1) The study of biology is not two disciplines, but one discipline; (2) biological science is not to be set over against physical science, but is to be included in it; (3) zoölogy, when presented under the name of biology, is not a sham biology, but a "restricted biology"; (4) the better fundamental division of biology is into general morphology and general physiology, not into botany and zoölogy. Stated thus, with such condensation as is necessary for clearness, it is hoped that the exact meaning of Mr. Herrick is preserved. These four points, only the third of which seems to have direct bearing on the question at issue, may now receive their proper attention.

¹ *Science*, Vol. XXI., p. 234. New York.

² Columbia College Circular of Information, 1893, Pt. VII., p. 4. New York.

³ Boston University Year Book, Vol. XX., p. 66, 1893. Boston.

⁴ *Science*, Vol. XXI., p. 220. New York.

(1) There is certainly a unity in the science of biology. This unity is not, however, zoölogy. Breadth of view demands rather a recognition of the true unity, and for such recognition the writer is contending. "Good observation" will convince Mr. Herrick that one who writes "Biology is either a superficial smattering of natural history facts and methods—and in this case not of any value—or a strong, uniform presentation of the facts of botany and zoölogy—and in this case a very different thing from a sham biology which is principally, or all, zoölogy"—doubtless appreciates the breadth of biological science almost, if not quite, as clearly as he would if contending that zoölogy alone may pass current for biology. For such higher unity of biology it is a duty to contend against any or all disintegrating views that may arise from the misfortune of a narrow education.

(2) Mr. Herrick laments the inadequacy of my early training along biological lines and, indeed, charges me in so many words with having been myself a student at Johns Hopkins University. As principal evidence of an indwelling incapacity he adduces my setting biological science over against physical science. He writes, regretfully reminiscent: "a student who had followed this general biological course with a fair degree of success, would have learned that 'biological science is not to be set over against physical science in the broadest sense,' but that in this broadest sense biology is a physical science coördinate with chemistry and physics." In this connection the following quotation may be noted. It is from Dr. C. O. Whitman, an acknowledged leader, I believe, in American zoölogy: "The term biology is so frequently used with latitudinarian disregard of its etymological significance that it becomes necessary to recall its original meaning. . . . As still used by the best authorities, the term is a very comprehensive one, denoting not one science or the fragment of a science, but a multitude of sciences embracing the entire organic world in contradistinction to the inorganic or physical world. From this broad standpoint all the natural sciences fall into two great groups, known as the biological and the physical."¹ Doubtless, no italicizing will be required to impress Mr. Herrick with the ripeness of the harvest that awaits his discriminating mission-effort among his biological colleagues. With his fine solicitude for those whose "early comprehensive training" has not sufficed to distinguish clearly between physical and biological science, he will scarcely permit himself to overlook so distressing a failure in Dr. Whitman to conform with the standard of absolute correctness. The writer, however, must continue to believe that a grouping of natural sciences into physical and biological sciences is not altogether unproductive of right thinking and ventures to commend, as a useful discipline, to Mr. Herrick, the reading of Dr. Whitman's programme not only on account of the value of its definition of biology and the general breadth of its views, but also because a uniform line of defence will be highly advantageous for all who find themselves, whether by necessity or by choice, enlisted under the flag of the sham biology.

(3) While the term "restricted biology" is an ingenious suggestion for such courses in zoölogy as are offered at Columbia College and Johns Hopkins University under the inappropriate name of biology, it is not clear that the old-established word "zoölogy" is not better. It is scarcely so vague and has the merit of brevity. If either of these institutions should gracefully announce a "department of Restricted Biology" and should confer degrees upon "doctors of philosophy in restricted biology" it would certainly indicate the dawn of ethical development if not the noon-tide of philological precision. And if such a consummation lies near the heart of Mr. Herrick he shall not wander farther without my sympathy. But, unfortunately, one must here note the crucial and deplorable fact; these institutions do not employ the term "restricted biology," but use instead the broader term, biology, for their zoölogical courses. Since a part of anything posing valiantly as the whole is universally recognized as a sham, it is hardly possible in such a case for the sham biology to escape its just characterization.

(4) It is unreasonable, of course, to ask that an American "biologist" should be familiar with the literature of plant-

morphology from Hofmeister to Guignard, Strasburger and Treub. But those humbler botanists that have followed the progress of recent investigation in this field would realize how distant seems to be the day when general homologies between higher plants and higher animals may be demonstrated with certainty. The established fact that between sporophytic plant-embryos and gamozoan animal-embryos there exist few known homologies in general must give pause to ambitious talk about a "general morphology." Such a general morphology would certainly demand a basis of general phylogenetic and ontogenetic comparison. It is true that in cytology, and especially in nuclear dynamics there may be read, for the future, possibilities of a general morphology. Chromatomes may indeed be always homologous as well as analogous, broadly speaking. But to-day "general morphology," as a science, does not exist. I should be glad to learn the title of some compendium of general morphology. I should be pleased to hear the name of some living or deceased investigator who could, in the broad sense of Mr. Herrick's division, be termed a "general morphologist." The fundamental division of biology into two sub-sciences, one of which, at least, does not exist as such, seems scarcely so productive of good as the time-honored division into botany and zoölogy. Between plant-physiology and plant-morphology there are innumerable series of contact-points. Between plant-morphology and animal-morphology there are few. Until, therefore, we may claim a far wider knowledge of the facts of morphology and physiology—at least in the field of botany—it will be difficult for Mr. Herrick to impose his divisions of biology to suit the terms of his argument.

In the second place, the science of biology is clearly not principally a method or discipline as Mr. Herrick seems to think it is; it is, also, and primarily, an orderly group of facts about an orderly group of things. These things are living things. The primary division must therefore be along the line of mass, not along the line of method. Living things conveniently divide with great exactness—although not absolutely, as Mr. Herrick acutely indicates—into plants and animals. Biology, therefore, divides conveniently into botany and zoölogy. A particular method is the essence of morphology, but plants in the aggregate are the essence of botany. Biology is, primarily, a group of facts about a group of things, not a group of facts about a group of methods of studying things. Plants (for example) are things, not methods, and therefore the fundamental division of biology into botany and zoölogy is more logical than its division into morphology and physiology. It thus appears not only that the divisions of biology urged by Mr. Herrick have never existed and do not exist now, but also that logically they should not exist as primary divisions but only as secondary. Finally, even if they did and should exist, the classification would not help the sham biology. For the union of a sham "general morphology" and a sham "general physiology" would probably result in a sham biology, and a "general morphology" which upon criticism reveals itself as the special morphology of animals is evidently a sham morphology.

It is a source of regret to the writer that anyone should suppose that he would "stigmatize" any university or any honorable graduate of a university. His function is purely indicative, and, while he agrees with Mr. Herrick that the truth about the state of affairs in certain curricula and the state of culture in certain graduates is so melancholy that perhaps even so strong a word as "offensive" may rightly be applied, he must disclaim any connection with such a condition beyond that of an interested spectator, grieved that able young men should be dwarfed in their conceptions of the great field of biology through acceptance of a sham in place of the truth. He has the kindest of feelings for such young men and a warm sympathy for institutions straining every nerve in an unequal struggle with others of greater wealth and breadth. But he cannot permit his sympathy and kindly feeling to withhold him from the task of pointing out to those who may profit, perhaps, the impossibility as well as the undesirability of further acceptance of shams for realities. If words mean anything, zoölogy and biology are not synonymous, and it is hoped that no false pride will prevent the zoölogists from

¹ Programme of Courses in Biology, 1892-93, p. 6. Chicago.

joining the botanists in the development of an accurate nomenclature. For while some sneer at nomenclature as a trivial matter and of no importance, it must be remembered that nomenclature is the expression of ideas, and ideas are of much importance.

CONWAY MACMILLAN.

University of Minnesota.

Photographs of Scientific Men.

A NOTE in your recent issue having to do with a request for the photographs of American botanists suggests that an appeal made through the columns of *Science* is likely to aid a collection made by myself. Some six or seven years ago, finding great difficulty in procuring the portraits of American scientists, I began gathering the photographs of the members of the National Academy of Science, and last year deposited in the Smithsonian Institution a collection of mounted portraits (with mounted autograph letters) of every member of our academy save two. This collection forms part, I believe, of the exhibit of the Smithsonian Institution at the Chicago Columbian Exhibition. The two portraits which are needed to make the set entirely complete are those of John Henry Alexander (1812-1867) of St. James College, Maryland, and later of the U. S. Coast Survey, and Jonathan Homer Lane (1819-1880), long connected with the U. S. Coast Survey and the U. S. Patent Office. I should be glad to obtain photographs of the two scientists or to make arrangements for the copying of any likeness of them known to exist.

MARCUS BENJAMIN.

640 Madison Avenue, New York City, May 18, 1893.

The Palæolithic Man in Ohio.

IN the second number of *The Journal of Geology*, Mr. Wm. H. Holmes has resumed his polemic against the evidence of the existence of palæolithic man in North America with a long article upon "Traces of Glacial Man in Ohio." Like his previous article upon the Trenton finds, this, too, is characterized by the kind of reasoning, which a correspondent of *Science* has called the argument *ad ignorantiam*, i. e., because he has failed to find palæolithic implements in a certain locality, therefore no one else has ever found them there. The present article, however, exhibits also a striking example of what might be called "the argument by monopoly." Mr. Holmes produces two fanciful cuts to show how the top of a gravel pit might have slid down so as to bury Indian relics coming from the surface; but he cannot see any sense in Professor Wright's preparing a plate to show precisely where in the same gravel-pit Mr. Mills actually found the object in dispute.

But the great difficulty about Mr. Holmes's discussion of this subject is that he has no correct appreciation of what a palæolithic implement really looks like. This is not to be wondered at when we reflect that his studies in "archæology" have been limited to investigations of the subject of "native art." He says "close analogies of form between Indian rejects and some varieties of European palæolithic objects are too common to permit the attachment of much value to this feature of this or any other similar find." Accordingly he proceeds to prepare a plate containing, besides the object discovered by Mr. Mills, of which he gives as good a copy as he can have made, four unfinished Indian celts found by him fifty miles away. Of these objects he says, "they correspond very closely in material and appearance with the New Comerstown specimen, as will be apparent from an examination of the plate. The figures are presented without identification in order that the student may, by an effort to distinguish them, convince himself of the similarity of the supposed paleolith to the quarry-shop rejects of the region."

Now I undertake to assert that any competent student of prehistoric archæology who has studied the subject in the Old World, where palæolithic implements have been found in large numbers, will have no difficulty in discriminating upon Mr. Holmes's plate between the true palæolithic implement and the four unfinished Indian celts placed beside it. All plates, however, fail to give a fair representation of solid objects like these, from the necessity of the case. They must be handled to be

understood. The four unfinished celts resemble those previously figured by Mr. Holmes in describing the objects he discovered near Washington, where I have myself found similar objects several years ago. I repeat here, what I have said in another place, "no trained archæologist would hesitate for a moment to pronounce that the objects figured in the article entitled 'A Quarry Workshop' (*American Anthropologist*, Vol. III., plate 4) do not bear the slightest resemblance to real palæolithic implements."

I conclude this note with what I have already urged to the readers of *Science*, that "only a jury of the acknowledged prehistoric archæologists of the world is competent to pronounce judgment upon this question."

HENRY W. HAYNES.

Boston, May 13, 1893.

BOOK-REVIEWS.

Mineral Resources of the United States. 1891. By DAVID T. DAY. Washington, D. C., Department of the Interior, Government Printing Office. 1893. 630 p.

It is somewhat unfortunate that these volumes cannot be more promptly produced, the late date of their issue impairing materially the value of the statistics contained. But in spite of this they are always welcome, and together—the present volume being the eighth in the series—they form a valuable component of every library. The arrangement is the same as in previous issues, and we find the familiar names of Birkinbine, Kirchoff, Weeks, Parker, and others under their respective specialties. Mr. Parker's statistical article on coal is exhaustive, occupying nearly 200 pages in all, and is supplemented by the articles on coke, petroleum, and natural gas by Mr. J. D. Weeks. Mr. Wm. C. Day continues his paper on stone from the "Resources" for 1889-90. An admirable and much-needed division appears upon the clay materials of the United States, written by Mr. Robert T. Hill, and as this is in some respects the feature of the present volume an outline may not be out of place. Beginning with descriptive remarks, Mr. Hill passes on to the commercial classification, the origin and natural classification, residual or rock kaolins, and sedimentary or bedded clays. The sedimentary clays of the geological formations are given in natural sequence. The accessory minerals used in the clay industries are described and then the occurrence of clay materials by States.

Other interesting articles are those on natural and artificial cements, by Spencer B. Newberry, both descriptive and statistical, on precious stones by the expert, Mr. Geo. F. Kunz, and Mr. Packard's descriptive article on aluminum, the last including several pages on bauxite, with analyses and a sketch of the development in the South. Alabama, Georgia, and Arkansas are mentioned as containing the mineral, but Tennessee with its good promise, Virginia, and North and South Carolina are not spoken of.

An unfortunate slip of the binder has placed pages 49-64 inclusive between pages 32 and 33, but in other respects the book is all that can be wished for.

C. P.

William Gilbert of Colchester, On the Loadstone and Magnetic Bodies. A translation by P. Fleury Mottelay. New York, John Wiley & Sons.

A RATHER acrimonious discussion between Professor S. P. Thompson and Messrs. Wiley & Sons has attracted even more attention to this book than it would otherwise have received. It will be remembered that the Gilbert Club was formed in England a few years ago, and that one of the objects of their existence was the publication by subscription of Gilbert's works. Professor Thompson was one of the committee on publication, and the matter seems to have been left mostly to him. From various causes, one of which was possibly the fact that the latter is translating and editing a number of books on his own account, the publication of the Gilbert Club has been delayed. Previous to the determination of the club to undertake the publication of Gilbert's work, Mr. Mottelay had been seized with the same idea, and, as neither he nor his publishers were in any way infringing on the rights of the Gilbert Club, the work has recently been

issued. This aroused the ire of Professor Thompson, who, not being able to find any fault with Mr. Mottelay, wrote a number of rather bitter letters to the various technical papers, in which he spoke very disparagingly of Messrs. Wiley & Sons, and their conduct in publishing a book he had intended to publish himself. He was so evidently in the wrong, however, that most of the papers refused to allow him space on the subject, and united in defence of the publishers of the book, and Professor Thompson himself has probably by this time seen his mistake.

Of the book itself there is nothing but praise to be said. Mr. Mottelay is a worker of no mean reputation in this line of work, and his notes are always interesting and instructive. The translation seems to have been well done, so far as can be judged by comparing a few passages of the original which have appeared with the book. Mr. Mottelay's acquaintance with the vocabulary of the Schoolmen is of great use to him in the work, not that Gilbert was a schoolman, very far from it, but the language of philosophy had only begun to get rid of their marks (if indeed it is entirely free to this day).

On reading the book, we are struck with the sturdy self-confidence of the man, Gilbert of Colchester. He was right, and he knew it. A little bit of this is due possibly to the age he wrote in, but even more, it seems, to the man. Fearless he is in drawing conclusions, and he does not hesitate to dispute the evidence of others when it does not agree with his theory. Yet in one instance only does he appear to have been mistaken, i.e., in his proposed method of finding longitude by the inclination of the compass, which he proposed under the idea that the inclination was constant.

A few extracts from the work will give a good idea of the man and his work.

Before doing so, we may mention the fact that Lord Bacon thought that Gilbert had carried his theory a little too far, and had said that Gilbert had "endeavored to build a ship out of materials not sufficient to form the rowing-pins of a boat."

Page 2. "But lest the story of the loadstone should be jejune, and too brief, to this one sole property then known were appended certain figments and falsehoods, which in early times no less than nowadays were by precocious sciolists and copyists dealt out to mankind to be swallowed. For example, they asserted that a loadstone rubbed with garlick does not attract iron nor when it is in presence of a diamond. The like of this is found in Pliny and in Ptolemy's "Quadripartitum," and errors have steadily been spread abroad and been accepted — even as evil and noxious plants ever have the most noxious growth — down to our day, being propagated in the writings of many authors, who, to the end that their volumes might grow to the desired bulk, do write and copy all sorts about ever so many things about which they know naught in the light of experience. Such fables about the loadstone even Georgius Agricola, a man that has deserved well indeed of letters, has inserted as truthful history in his books, 'De Natura Fossilium,' putting his trust in others' writings."

Page 102. "In Baptista Porta's opinion there seems to be a mixture of stone and iron, i.e., ferruginous stone, or stony iron. 'The stone,' he says, 'is not changed into iron so as to lose its own nature, nor is the iron merged in the stone, but that it retains its own essence; and while each strives to overcome each, from the struggle results the attraction of the iron. In the mass of the loadstone there is more stone than iron; therefore the iron, lest it should be dependent on (subdued by) the stone, craves the strength and company of iron, to the end that what it cannot procure of itself it may obtain by the help of the other. The loadstone does not attract stones, because it has no need of them; and if one loadstone attracts another, it is not for the sake of the stone, but of the iron shut up in the stone.' As though the iron in a loadstone were a distinct body, and not blended with one another like all other metals in their ores. And it is a height of absurdity to speak of these substances thus confounded together as warring with each other, and quarreling, and calling out from the battle for forces to come to their aid. Now iron itself when

CALENDAR OF SOCIETIES.

Agassiz Scientific Society, Corvallis, Ore.

May 10. — Professor Dumont Lotz, Food Adulterants.

Biological Society, Washington.

May 20. — V. A. Moore, the Distribution of Pathogenic Bacteria in the Upper Air Passages of Domesticated Animals; C. V. Riley, Some Further Notes on Yucca Pollination; B. W. Evermann, The Ichthyologic Features of the Black Hills; W. H. Dall, New Forms of Fossils from the Old Miocene of the Gulf States; C. Hart Merriam, Biology in our Colleges; C. Hart Merriam, Facts of General Biological Interest Resulting from a Study of the Kangaroo Rats.

Geological Society, Washington.

May 24. — Whitman Cross, On the Occurrence and Characteristics of Laccolitic Rocks; Walter H. Weed, The Northern Peaks of the Crazy Mountains, Montana.

INDEX TO VOLUME XVIII OF SCIENCE

is in preparation, and will be issued at an early date.

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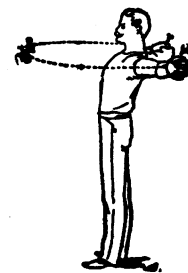
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touched with loadstone seizes iron with not less force than loadstone itself. These fights, seditions, conspiracies, in a stone, as though it were nursing quarrels as an occasion for calling in auxiliary forces, are the maunderings of a babbling hag, rather than the devices of an accomplished prestigiator."

So much for his attacks on the older philosophy. As an example of his own reasoning, we may give the following: "What is it that produces this movement? (speaking of the attraction of electrified bodies). The body itself circumscribed by its contour? Or is it something imperceptible for us, flowing out of the substance into the ambient air? And, if it is an effluvium, does the effluvium set the air in current, and is the current then followed by the bodies? Or is it the bodies themselves that are directly drawn up? But, if the amber attracts the body itself, then suppose the body itself is clean and free from adhesions, what need is there of friction? Nor does the force come from the lustre proceeding from the rubbed and polished electric, for the vincentina, the diamond, and pure glass attract when they are rough, but not so strongly nor so readily; because they are not so readily cleaned from the extraneous moisture settled on the surface, nor are they subjected all over to such an equal degree of friction as to be resolved into effluvia. Nor does the sun, with its shining and its rays, which are of vast importance in nature, attract bodies thus, and yet the common run of philosophers think that liquids are attracted by the sun, whereas only the denser humors are resolved into rarer, (and) into vapor and air; and thus, through the motion given them by diffusion, they ascend to the upper regions, or, being attenuated exhalations, they are lifted by the heavier air. Neither does it seem that the electric attraction is, by the effluvia, rarefying the air so that the bodies, impelled by the denser air, move towards the source of rarefaction. If that were so, then hot bodies and flaming bodies would attract other bodies, but no lightest straw, no rotating pointer is drawn toward a flame. If there is afflux and appulsion of air, how can a minute diamond, the size of a chick-pea, pull to itself so much

air as to sweep in a corpuscle of relatively considerable length, the air being pulled toward the diamond only from round a small part of one or other end? Beside, the attracting body must move more slowly or stand still before coming into contact, especially if the attracting body be a broad flat piece of amber, on account of the heaping-up of air on the surface, and its rebounding after collision. And if the effluvia go out rare and return dense, as with vapors, then the body would begin to move towards the electric a little after the beginning of the application, yet when rubbed electrics are suddenly applied to a versorium, instantly the pointer turns, and the nearer it is to the electric, the quicker is the attraction. . . . In addition to the attraction of bodies, electrics hold them for a considerable time, hence it is probable that amber exhales something peculiar which attracts the bodies themselves, and not the air. It plainly attracts the body itself in the case of a spherical drop of water standing on a dry surface; for a piece of amber held at a suitable distance pulls towards itself the nearest particles and draws them into a cone, were they drawn by the air the whole drop would come toward the amber."

Page 273. "The variation in the Indian Ocean all the way to Goa and the Moluccas is noted by the Portuguese, but they are mistaken in many points, for they follow the first observers who set down the variations for sundry places, ascertained by the use of unfit instruments, or by inaccurate observations, or by conjecture. Thus in the island of Brandô they make the compass vary 22 degrees to the northwest. Now, in no region, in no place on the earth that has not a higher latitude than that, is the variation so much as 22 degrees; in fact the variation on the island is trifling, so when they say that in Mozambique the compass varies to the northwest one point, they are in error, even though the compass they use be that of Portugal, for without a doubt the needle varies in Mozambique to the southwest one-quarter of a point or more."

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R. A. F.

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Texas Plants. I will collect sets of plants represented in this region of Texas, either for sale or for exchange for literature, for anyone who will send me an order in time to fill it. Plants to be ready complete in November. C. F. Maxwell, Box 174, Dublin, Texas.

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The undersigned has skins of Pennsylvania and New Jersey birds, as well as other natural history specimens: which he wishes to exchange for marine, fresh water, and earthworms of the South and West. Correspondence with collectors desired. J. Percy Moore, School of Biology, University of Pennsylvania, Philadelphia.

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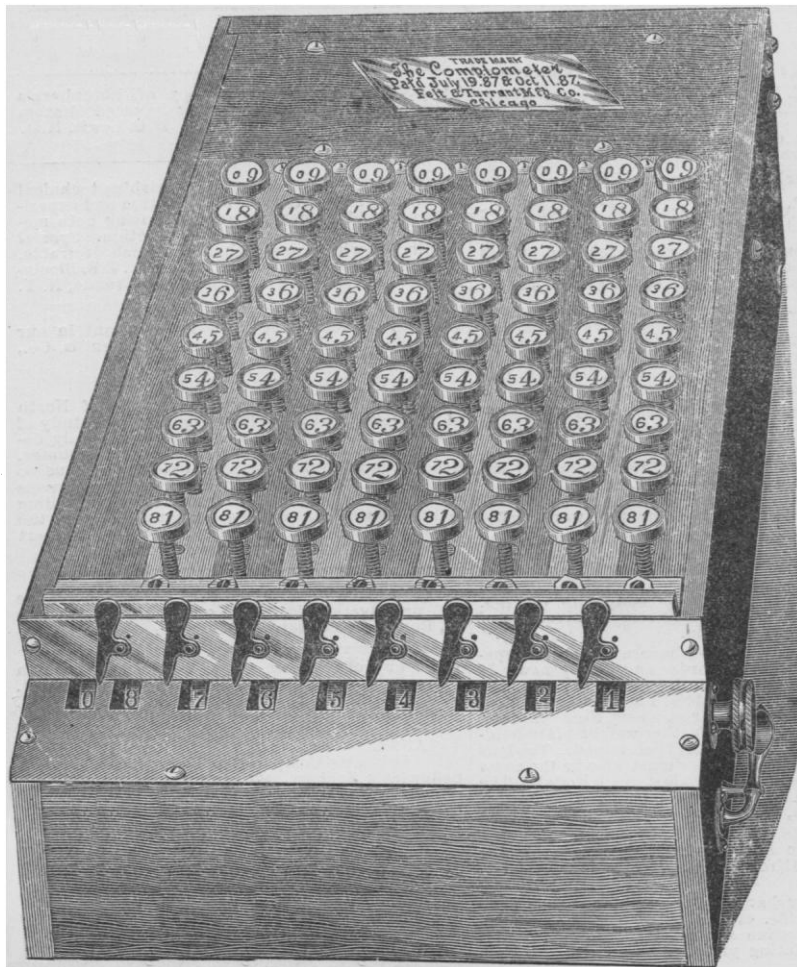
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